VIII. *On the noxious Quality of the Effluvia of putrid Marſhes. A Letter from the Rev. Dr.* Prieſtley *to Sir* John Pringle.

To Sir JOHN PRINGLE, Baronet.

DEAR SIR,

Redde, Dec. 16, 1773. HAVING purſued my experiments on different kinds of air conſiderably farther, in ſeveral reſpects, than I had done, when I preſented the laſt account of them to the Royal Society; and being encouraged by the favourable notice which the Society has been pleaſed to take of them, I ſhall continue my communications on this ſubject; but, without waiting for the reſult of a variety of proceſſes, which I have now going on, or of other experiments, which I propoſe to make; I ſhall, from time to time, communicate ſuch detached articles, as I ſhall have given the moſt attention to, and with reſpect to which, I ſhall have been the moſt ſucceſsful in my inquiries.

Since the publication of my papers, I have read two treatiſes, written by Dr. ALEXANDER, of Edinburgh, and am exceedingly pleaſed with the ſpirit

of philoſophical inquiry, which they diſcover. They appear to me to contain many new, curious, and valuable obſervations; but one of the concluſions, which he draws from his experiments, I am ſatisfied, from my own obſervations, is ill founded, and from the nature of it, muſt be dangerous. I mean his maintaining, that there is nothing to be apprehended from the neighbourhood of putrid marſhes.

I was particularly ſurprized, to meet with ſuch an opinion as this, in a book inſcribed to yourſelf, who have ſo clearly explained the great miſchief of ſuch a ſituation, in your excellent treatiſe on the diſeaſes of the army. On this account, I have thought it not improper, to addreſs to you the following obſervations and experiments, which I think clearly demonſtrate the fallacy of Dr. ALEXANDER's reaſoning, indiſputably eſtabliſh your doctrine, and indeed juſtify the apprehenſions of all mankind in this caſe.

I think it probable enough, that putrid matter, as Dr. ALEXANDER has endeavoured to prove, will preſerve other ſubſtances from putrefaction; becauſe, being already ſaturated with the putrid effluvium, they cannot readily take any more; but Dr. ALEXANDER was not aware, that air thus loaded with putrid effluvium is exceedingly noxious when taken into the lungs. I have lately, however, had an opportunity of fully aſcertaining how very noxious ſuch air is.

Happening to uſe at Calne, a much larger trough of water, for the purpoſe of my experiments, than I had done at Leeds, and not having freſh water

ſo near at hand as I had there, I neglected to change it, till it turned black, and became offenſive, but by no means to ſuch a degree, as to deter me from making uſe of it. In this ſtate of the water, I obſerved bubbles of air to riſe from it, and eſpecially in one place, to which ſome ſhelves, that I had in it, directed them; and having ſet an inverted glaſs veſſel to catch them, in a few days I collected a conſiderable quantity of this air, which iſſued ſpontaneouſly from the putrid water; and, putting nitrous air to it, I found that no change of colour or diminution enſued, ſo that it muſt have been, in the higheſt degree, noxious. I repeated the ſame experiment ſeveral times afterwards, and always with the ſame reſult.

After this, I had the curioſity to try how wholeſome air would be affected by agitation in this water; when, to my real ſurpriſe, I found, that after one minute only, a candle would not burn in it; and, after three or four minutes, it was in the ſame ſtate with the air, which had iſſued ſpontaneouſly from the ſame water.

I alſo found, that common air, confined in a glaſs veſſel, in contact only with this water, and without any agitation, would not admit a candle to burn in it after two days.

Theſe facts certainly demonſtrate, that air which either ariſes from ſtagnant and putrid water, or which has been for ſome time in contact with it, muſt be very unfit for reſpiration; and yet Dr. ALEXANDER'S opinion is rendered ſo plauſible by his experiments, that it is very poſſible that many perſons may be rendered ſecure, and thoughtleſs

of danger, in a ſituation in which they muſt neceſſarily breathe it. On this account, I have thought it right to make this communication as early as I conveniently could; and as Dr. ALEXANDER appears to be an ingenuous and benevolent man, I doubt not but he will thank me for it.

That air iſſuing from water, or rather from the ſoft earth, or mud, at the bottom of pits containing water, is not always unwholeſome, I have alſo had an opportunity of aſcertaining. Taking a walk, about two years ago, in the neighbourhood of Wakefield, in Yorkſhire, I obſerved bubbles of air to ariſe, in remarkably great plenty, from a ſmall pool of water, which, upon inquiry, I was informed had been the place, where ſome perſons had been boring the ground, in order to find coal. Theſe bubbles of air having excited my curioſity, I preſently returned, with a baſon, and other veſſels proper for my purpoſe, and having ſtirred the mud with a long ſtick, I ſoon got about a pint of this air; and, examining it, found it to be good common air; at leaſt a candle burned in it very well. I had not then diſcovered the method of aſcertaining the goodneſs of common air, by a mixture of nitrous air. Previous to the trial, I had ſuſpected that this air would have been found to be inflammable.

I ſhall conclude this letter with obſerving, that I have found a remarkable difference in different kinds of water, with reſpect to their effect on common air agitated in them, and which I am not yet able to account for. If I agitate common air in the water of a deep well, near my houſe in

Calne.

Calne, which is hard, but clear and ſweet, a candle will not burn in it after three minutes. The ſame is the caſe with the rain water, which I get from the roof of my houſe. But in diſtilled water, or the water of a ſpring-well near the houſe, I muſt agitate the air about 20 minutes, before it will be ſo much injured. It may be worth while, to make farther experiments, with reſpect to this property of water.

In conſequence of uſing the rain water, and the well water above-mentioned, I was very near concluding, contrary to what I have aſſerted in my printed papers, that common air ſuffers a decompoſition by great rarefaction. For when I had collected a conſiderable quantity of air, which had been rarefied about four hundred times, by an excellent pump made for me by Mr. SMEATON, I always found, that when I filled my receivers with the water above-mentioned, though I did it ſo gradually as to occaſion as little agitation as poſſible, a candle would not burn in the air that remained in them. But when I uſed diſtilled water, or freſh ſpring water, I undeceived myſelf.

I think myſelf honoured by the attention, which, from the firſt, you have given to my experiments, and am, with the greateſt reſpect,

Dear Sir,

Your moſt obliged

humble Servant,

London, 7 Dec. 1773.

J. PRIESTLEY.

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POSTSCRIPT.

I cannot help expreſſing my ſurprize, that ſo clear and intelligible an account, of Mr. SMEATON's air-pump, ſhould have been before the public ſo long, as ever ſince the publication of the forty-ſeventh volume of the Philoſophical Tranſactions, and yet that none of our philoſophical inſtrument makers ſhould attempt the conſtruction. The ſuperiority of this pump, to any that are made upon the common plan, is, indeed, prodigious. Few of them will rarefy more than 100 times, and, in a general way, not more than 60 or 70 times; whereas this inſtrument muſt be in a poor ſtate indeed, if it do not rarefy 200 or 300 times; and when it is in good order, it will go as far as 1000 times, and ſometimes even much farther than that; beſides, this inſtrument is worked with much more eaſe, than a common air-pump, and either exhauſts or condenſes at pleaſure. In ſhort, to a perſon engaged in philoſophical purſuits, this inſtrument is an invaluable acquiſition. I ſhall have occaſion to recite ſome experiments, which I could not have made, and which, indeed, I ſhould hardly have dared to attempt, if I had not been poſſeſſed of ſuch an air-pump as this. It is much to be wiſhed, that ſome perſon of ſpirit in the trade would attempt the conſtruction of an inſtrument, which would do great credit to himſelf, as well as be of eminent ſervice to philoſophy.